SCIENCE

FRIDAY, APRIL 3, 1914

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THE EVOLUTIONARY CONTROL OF ORGANISMS AND ITS SIGNIFICANCE[1]

I

A COMPARATIVELY brief period has passed since the evidence brought together by Darwin in connection with the results slowly accumulated from other sources has clearly demonstrated that the diversity of organic life in the world occurs through evolution. It is one thing, however, to clearly diagnose a condition and quite another to understand the causes which have brought about the phenomenon so that similar results may be produced advantageously. With the assumption that evolution was merely the survival of those forms which were best adapted to the environment, generation after generation, the explanation of the method as well as its practical application, namely the improvement of organisms in any given direction, was apparently a simple matter. It seemed evident that man had modified and adapted to his welfare various plants and animals by a more or less unconscious and haphazard selection long before history records civilization.[2] Why then could not civilized man carry forward the work and with the knowledge gained since the principles of evolution were recognized, obtain far-reaching results within a brief period of time. All that seemed necessary was to have individuals

[1] Presidential address before the twenty-third annual meeting of the Ohio Academy of Science, Oberlin, O., November 28, 1913.

[2] One need not be a pessimist to assert the actual evidence thus far obtained indicates that the supposed progress made in the improvement of domesticated animals and plants is nothing more than the sorting out of pure lines and thus represents no advancement.

of a particular organism in large numbers, and by continued selection of the variations best meeting the conditions move rapidly forward by a series of increments toward the goal of perfection. What could be more simple? Instead of corn having an acreage yield of fifty bushels, there would with a proper supply of plant food be a production of two hundred, two hundred and fifty or even three hundred bushels. Instead of politicians with no perspective beyond their immediate welfare—a reelection—instead of college presidents and faculties with their numerous shortcomings—according to the students and occasionally the trustees—there would be the ideal individual bred to specification and not necessarily made in Germany.

Unfortunately, variations with a perverseness incomprehensible uniformly refused to accumulate in the manner desired and at times even demonstrated their obstinacy by retrogression. It was plainly evident that there were limits imposed by nature not easily passed, and in connection with which much experimental work must be undertaken before definite progress was made and the facts fully understood.

With a realization of the difficulties involved in an attempt to apply evolution, it will be well to pause for a moment and consider certain fundamental principles before discussing the results of some of the investigations which for a time at least promised much toward the solution of the problem. Thus it may be stated that evolution in its different modifications postulates in general (1) the occurrence of numerous varying individuals, some of which are (2) eliminated by environmental stimuli leaving few or no offspring, while (3) the survivors transmit to their progeny the characters which proved of selective value, with the result that (4) through the continuation of the process the race eventually becomes adapted to surrounding conditions. The first two propositions are merely statements of fact. The real difficulties of the situation are those of ascertaining how variations which are transmitted may be recognized and produced so that the result will be a cumulative one. Until this is done breeders must continue to proceed in the same haphazard manner that they have followed for countless generations.

By selecting the largest and most perfect ears of seed corn from the variations present in the field, conversely eliminating the remainder from reproducing, the corn grower plants with a fatuous trust in providence that a crop somewhat better or at least as good as the preceding crop will be produced. If it is a type comparatively pure the average may be maintained and the hope partially realized, but the chances for retrogression are far greater than for advancement, inasmuch as there is no means for distinguishing a variation which will be transmitted with equal or better results than in the preceding generation, from one that represents a fluctuation due to nurture and which is non-transmissible. Thus the apparently inferior ear of corn will frequently produce a yield far better than obtained from one which is perfection as graded by the methods of the "corn show," and if from the same pure race, the resultant crop will be at least as good. Artificial methods of hybridization, which furnish an immediate advancement in the succeeding generation, result in a gain which is only temporary. The increased stimulus to growth vanishes as a fluctuation.

Thus it is quite evident that there exists a problem in the evolutionary control of organisms even the partial solution of which will mark an extraordinary advancement not only for agriculture, horticulture,

and animal breeding, but also for society in general.

II

The general results of the investigations bearing upon the evolutionary control of organisms may be grouped around the principles of Mendelism, the mutation theory, and pure line breeding.

The rediscovery in 1900 of the fundamental laws governing hybridization so brilliantly established by Mendel in 1865, but unfortunately concealed in the obscure publications of the Natural History Society of Brunn, opened an extraordinary field for experimental work. This has already developed to vast proportions in connection with both the results obtained and the speculations involved, while the end is not in sight.

The investigations of Mendel, now so familiar to all biologists, and which may be mentioned somewhat in detail here because of their bearing on mutation, consisted primarily in the crossing of tall and dwarf peas, with the result that the first filial (F_1) or hybrid generation consisted entirely of tall plants. When, however, seeds from these plants were sown the ratio of tall to dwarf plants became 3 to 1 in the second (F_2) hybrid generation, a result explained by the theory of dominant and recessive characters on the basis that there are certain determiners of unit characters in the germplasm which dominate over others during the development of the somatoplasm or body of the individual in the higher forms of life. More recently the presence and absence theory has been applied in interpreting the results. In a manner similar to the preceding when smooth yellow peas were crossed with wrinkled green peas the first hybrid generation consisted of smooth yellow forms inasmuch as the character smooth and the character yellow were dominant over the character wrinkled and the character green, and the crosses were known as dihybrids, inasmuch as they differed in respect to two characters. In the second hybrid generation the resultant ratio was 15 to 1 pure recessive, *i. e.*, wrinkled green, although the fifteen consisted of smooth yellow, smooth green, and wrinkled yellow in the proportion of 9 : 3 : 3. In the same way trihybrids have the ratio 63 to 1 pure recessive while any polyhybrid differing in "n" characters which mendelize in the usual manner will give an expected ratio of $4^n - 1$ to 1 pure recessive, which will become apparent only through the breeding of large numbers of individuals.

While the preceding summary represents the normal results in connection with the segregation of unit characters, studies of the past few years have demonstrated that many interesting relationships may occur between the factors governing the production of characters. For example, it has been found that two or more determiners are often present either of which will produce the given character as Nillson-Ehle demonstrated in hybrids of brown and white chaffed wheat, while on the other hand two or more determiners acting together may be necessary to bring about an effect. Such a condition exists, as Bateson in 1910 showed, in certain white-flowered sweet peas which when crossed produce purple flowers in the first hybrid generation. The results which have led to the theory of coupling and of repulsion, particularly the latter, where the expectancy of a pure recessive may be one among many thousands, go far toward suggesting a possible explanation of many so-called mutations on the basis of ancestral individuals heterozygous for one or more characters.

Do the Mendelian principles assist us, however, in attaining the goal which we are seeking, namely the building up of an ideal

organism which will continue to transmit its characters? The answer must be in the negative so far as the originating of anything new is actually concerned. Recessives may be obtained. Characters may be redistributed. They were present in the forms first utilized, however.

The mutation theory formulated by De Vries in 1901 approximately at the time interest was being awakened by the rediscovery of the hybridization principles of Mendel, needs no extended explanation to those who have been interested in evolution. Based on cultural experiments with *Œnothera lamarckiana,* one of the evening primroses, the appearance of relatively small numbers of forms which were quite distinct from the parental species and which bred true in subsequent generations, led to the inference that evolution had in many cases proceeded by discontinuous variations or mutations.

Long series of breeding experiments followed in connection with other organisms, both plants and animals, with results quite similar to those obtained by De Vries. Investigations were also made (Fischer, MacDougal, Tower, etc.) where organisms were subjected to stimuli abnormal in their nature, with the result that a modified progeny was obtained which bred true to the apparently induced character in succeeding generations. Furthermore, cytological studies (Gates, etc.) demonstrated some interesting relationships so far as differ- "mutants" were concerned.

While the evidence is far too insufficient to allow more than a tentative opinion, there are several conclusions concerning mutation which appear justified. The nature of the results obtained through the various agencies make it quite evident that they are not all due to a single underlying principle. There are many "mutants" the origin of which is most certainly to be explained on the basis of a heterozygous condition of the gametes, and much evidence has accumulated that *O. lamarckiana* of De Vries on which the mutation theory was founded belongs to this class. Furthermore there are mutants developing in connection with the action of abnormal stimuli although it is not at all improbable that some of these result from heterozygotes. It may be mentioned that Humbert (1911) in experiments with 7,500 pure line plants of *Silene noctiflora,* one of the "pinks" utilizing methods similar to those of MacDougal, failed to obtain any "mutants." Another explanation of the results in connection with the influence of abnormal stimuli is that the modification takes place through the destruction of a factor and thus the process is one of subtraction instead of addition. There are also investigations, notably those of Gates, in which the aberrant organism apparently results from the abnormal behavior of the chromosomes at some stage during the life cycle. *Œnothera gigas* with its tetraploid chromosomes is here of much interest.

Notwithstanding these diverse results, there is little indication that anything actually new has been added to the organism which would not have occurred within a pure line. If this is true the heterogeneous school of mutationists can be of little assistance beyond suggesting the way in which evolution did not take place.

The experiments on the basis of pure line breeding belong to a comparatively recent period and are of the utmost importance. Johannsen in 1903 published results based on a pure line of beans self-fertilized for successive generations and evidently homozygous. From a bean weighing 95 centigrams and far above the average in size he obtained plants producing beans varying in weight from approximately 35 to 70 centigrams, but all far below the weight of the parent. Utilizing

these in turn as parental forms, from those having a weight of 35–40 centigrams there resulted a progeny with an average of 57.2 centigrams, while from those having a weight of 65–70 centigrams a progeny was obtained which had an average of 55.5 centigrams. In other words, selection had not only failed to make any advancement, but actually resulted in a slight retrogression. Facts quite in accord with this but giving much more pronounced results have been obtained by Tower (1906), Jennings (1908), Johannsen (1909) and others. It should be noted, however, that there have been several experiments, notably those of De Vries with buttercups, Tower with potato beetles, and Smith with Indian corn, where a possible advance of a character was recorded in a group. Heterozygotes here may have been responsible for the result, although again the explanation may consist in the elimination of the effects of a determiner.

The results in mixed races as exemplified by corn, beans, etc., where selection has gradually improved a group of organisms but finally reached a limit beyond which no progress appeared possible, are comparatively well understood and are due, as explained by Shull (1908), to the separation of the pure lines which were present in the race at the beginning. This is where the average agriculturist, horticulturist, and animal breeder has gone far astray and, having succeeded for a few generations in making progress, has failed to understand why he may not continue to be successful.

Thus we find that attempts to modify a character by selection within pure lines within a small number of generations have almost universally failed, and that the few apparent results to the contrary must be looked upon with the suspicion that the population was a mixed race and that Mendelian principles applied.

Once again we are led to propound with still greater emphasis the question, "How then has evolution taken place?" "In what manner have organisms acquired their characters?" "Is it possible to escape the difficulties that confront the investigator on every side?"

III

The application of statistical methods to problems of biology has provided and will continue to provide facts of decided value obtainable in no other way. Nevertheless, the use of data "en masse" uncoordinated with experimental methods can not solve the riddle of existence so easily as some, at an earlier period at least, would have had us believe. There are, however, investigations which seem fundamental to the problem under discussion and which may well be approached from the statistical side. These relate to the influence of certain factors composing the environment as well as to the part played by asexual and sexual reproduction, corresponding in reality to close and cross breeding, upon variability and size in organisms.

Some studies undertaken in 1900 in connection with the influence of food supply on variability[3] based upon the comparison of groups of *Chrysanthemum leucanthemum* L., the common white daisy, as well as *Perca flavescens* Mitch., the yellow perch, indicated that the difference in variability as evinced by the coefficient of variation for a group with a maximum food supply as compared with a group having a minimum food supply, was extremely small and well within the limits allowed by the probable error. From this the inference was that external stimuli played an extremely unimportant part under normal conditions as a cause producing variability in general.

Attempts were subsequently made to ob-

[3] SCIENCE, p. 728, 1907.

tain data bearing on the results of close breeding and cross breeding which differ merely in degree from parthenogenesis and amphimixis. The question is an important one, for if cross breeding is only valuable in sorting out and combining existing characters, it not only obscures the facts, a knowledge of which is necessary before progress can be made in building up new characters, but results in no actual advancement in cumulative evolution. Here the material for study consisted of scalariform or cross-bred and lateral or close-bred (parthenogenetic) zygospores—in reality the young individuals—of the common filamentous green alga *Spirogyra inflata* (Vauch). Upon applying statistical methods the close-bred zygospores were found to be 23 per cent. more variable[4] in size as well as larger, both in length and actual volume, than the cross-bred zygospores. The results were not in accord with the general belief that cross breeding increased variability, although studies by Warren, Kellogg, Casteel and Phillips had pointed out that this belief was not substantiated by facts, which, however, did not actually warrant the idea that variability was decreased in cross-bred forms. The studies on the zygospores also suggested that sex existed primarily for the purpose of limiting variability, a hypothesis proposed on purely theoretical grounds by Hatschek in 1887. Another conclusion which followed from the same investigation was that in connection with the origin of death[5] and which may be mentioned here. This is summarized by stating that death apparently occurs as the result of the continually forming body cells becoming so variable through absence of control by amphimixis, that eventually some one group of functional importance fails to meet the limits imposed by the environment. In consequence of this the group, together with the remainder of the colony—the individual—perishes.

In connection with the difference in the variability of close-bred and cross-bred zygospores it seems quite evident that the result is brought about by some factor other than the environmental stimuli which are assumed to produce fluctuation, inasmuch as the material was homogeneous in every respect with the exception of the manner of reproduction. The question is a difficult one, however, not to be settled by a single investigation giving positive results, and because of its importance should receive attention.

In reference to those who hold to the belief that cross breeding, conjugation and amphimixis—the three terms differ merely in degree—increase variability, it may be well to inquire concerning some of the evidence which has been instrumental in formulating the opinion. Without any desire to be critical and at some risk of exceeding the controversial bounds which a paper of this nature allows, a few of the more important investigations touching upon the subject will be considered.

Castle, Carpenter, Clark, Mast and Barrows (1906) in a series of observations as to the effect of cross breeding and close breeding on the variability and fertility of the small fruit fly, *Drosophila ampelophila* Loew., stated that "inbreeding did not affect the variability in the number of teeth on the sex comb of the male, nor the variability in size," basing the opinion on the coefficient of variation in the number of spines and the standard deviation in the length of the tibia. In the former case the data certainly do not permit a clear conclusion one way or the other, but the value of the character which represents the sum of the teeth of the sex combs of the right

[4] Science, p. 907, 1908.

[5] Science, p. 935, 1912.

and left proximal tarsal segment, where there is undoubtedly correlation, may be open to objection under any consideration. If, however, from the data presented in the study the value of the coefficient of variation is computed, which, strange to say, was not done in the paper, and thus allowance made for the greater length of tibia in the cross-bred forms, the combined inbred forms exhibit a variability relatively 68 per cent. greater than the cross-bred forms.

Jennings (1911) in summarizing breeding experiments with *Paramecium* concluded that "The progeny of conjugants are more variable in size and in certain other respects than the progeny of the equivalent non-conjugants," and farther, "Thus conjugation increases variation." Continuing the investigations, he subsequently stated (1913) that conjugation increased the variation in the rate of reproduction. While the careful methods used by Jennings have brought to light many interesting and valuable facts, it is evident, from a critical consideration of the data, that they by no means allow such conclusions.

So far as size is concerned in a pure race, non-conjugants and their progeny were more variable than conjugants and their progeny, as noted in Table No. 28. In a wild race the progeny of the conjugants were slightly more variable than the progeny of the non-conjugants, as illustrated in Table No. 32, although in two of the nine generations tabulated the variability was greater in the case of the non-conjugants. So far as the rate of fission is concerned, the evidence is unmistakable that the conjugants were more variable. There is, however, a comparatively simple explanation for this when the statement is noted that the number of abnormal individuals, as well as the mortality, was greatest among the progeny of the conjugants. With a considerable number of forms thus having a lower rate of fission, one could expect nothing except a greater variability in the rate of fission. This becomes the more evident when it is found that the higher variability of the conjugants was caused by the considerable number with the low rate of fission.

Considering the data obtained in the breeding of plant forms where the assumption has long been prevalent that hybridization increases variability, it is found that the variability of the F_2 generation as compared with the F_1 generation or a single parental generation may be increased, but that the actual variability as a whole is not increased when the united parental types are taken into account. This may be illustrated by utilizing data from an interesting paper by Hayes (1912) dealing with correlation and inheritance in tobacco. Here, calculating the constants for two parental types combined (401 and 403) in respect to number of leaves and height of plant, it is found that the coefficient of variation has decidedly decreased through the hybridization, although the number of combinations have increased.

There exists the possibility, however, that variability will appear to be increased when forms having the same phenotype but different genotypes are bred together. Such a condition may be illustrated by the two white strains of sweet peas crossed by Bateson which produced purple flowers in the first (F_1) hybrid generation, and purple, pink, mixed, and white flowers in the second (F_2) hybrid generation. New combinations occur, but there is no evidence of increase in unit characters, nor is there an actual increase in variability.

Turning for a moment to size characters, the influence of cross breeding or conjugation is of decided interest inasmuch as facts bearing on the solution of the problem as to how size may be increased to the phys-

iological limit, even though the results hold for a single generation, have the greatest practical value for the future of agriculture and animal breeding.

It should first be noted that size in a unicellular organism is dependent on the absolute size of the individual cell with a limit undoubtedly imposed by laws governing the ratio between volume and surface in connection with osmosis. In multicellular organisms, however, size characters may depend upon either the size or the number of the component cells or upon both factors. This distinction possibly explains an apparent diversity in results obtained in the two groups.

Darwin, Mendel and others who have seriously considered the question have recognized that hybrids, among plant forms in particular, usually grew to a larger size than either parental form, a result probably due to the increased rapidity of cell division and consequently greater number of cells as conjectured by East. In the study of zygospores of *Spirogyra* it was therefore noticed with some interest that the cross-bred forms were smaller than the close-bred forms so far as both length and volume were concerned. Jennings (1911) in his study of *Paramecium* reached a contrary conclusion, stating that "The progeny of conjugants . . . were a little larger than the progeny of non-conjugants and the difference appears to be significant." This is correct merely in reference to length, however, and that it is not true for actual size as indicated by volume is evident on applying the formula for the volume of a prolate spheroid ($V = 1/6\pi l d^2$) by which it may be demonstrated that the non-conjugant forms, while smaller than the others at the beginning of the experiment, actually became larger. Thus in agreement with the zygospores of *Spirogyra*, conjugation decreased size.

The question immediately occurs as to the cause of the increased size and vigor among cross-bred multicellular organisms when the evidence indicates that cross-bred unicellular forms are smaller instead of larger. Some investigations that I have undertaken indicate an answer apparently meeting the conditions. While sufficient control experiments have not been made to venture more than a provisional opinion, the data suggest that the cells of cross-bred multicellular organisms are actually smaller than the cells of inbred or pure line forms, and that the more rapid division is a function of the greater ratio surface has to volume in a small cell with the better opportunity thus obtained for increased metabolism.

That there is need of further investigation on size and variability in pure lines and in cross-bred forms through the application of statistical methods in connection with the maintenance of pedigrees through long series of generations seems evident. Eventually theories will make way for facts which will allow a proper perspective.

IV

Where do the results presented in the preceding pages lead us? Does their value, so far as their bearing upon the production of new and transmissible characters that will build up an organism in a required direction, consist merely in the formulating of hypothesis after hypothesis which as investigations proceed will in turn make way for other hypotheses equally transient? Or, on the other hand, do they mark a definite progress along the lines we are endeavoring to follow, namely, the control of evolution.

Before attempting a reply which must prove more or less unsatisfactory to those looking forward to immediate results, it seems advisable to pause for a moment and in the light of the preceding discussion con-

sider the types of differences—variations—which exist in so far as they may effect the result with which we are chiefly concerned.

Beginning at an early period in the history of evolution with the idea that all variations might be inherited, results soon suggested that the characters due solely to surrounding influences such as food supply, etc., were not thus transmitted. These were called *fluctuating variations*. On the other hand, variations due to the structural changes in the germ cells which were passed on from one generation to another have been spoken of as *inherited variations*.

The evidence at present indicates that farther subdivisions must be made and that normal inherited variations consist of two quite distinct classes. The variations where the results are due to the interaction of factors in accordance with Mendelian principle, and which, adapting a term used by Plate (1913), may be called *amphimutations* inasmuch as the condition is due to the mingling of two lines of descent, the other variations, as a class, in which the results —evolution in the abstract—are due to a series of units added as increments, may well be called *cumulations*. It is quite evident that the term "mutation" can not continue to include both types. As a coordinate term fluctuating variations may be spoken of as *fluctuations*.

Under abnormal variations must be classified forms ranging from monstrosities to slight departures from the ordinary condition, some of which are undoubtedly due to the losses or modifications of unit characters through the action of extraordinary stimuli, while others may be due to abnormal and unequal distribution of chromosomes occurring at the time of their division. The *idiomutations* of Plate are here included.

The answer to the question as to the progress made in the application of evolution to the creation of new forms rests in the statement that the attack on the problem is becoming more concentrated. The selection of fluctuations has been tried and has failed. Efforts by means of amphimutations end in a maze of circles with no evident progress. *Idiomutations,* so far as one may judge from the evidence, present retrogression rather than advancement. It is by means of pure lines under normal conditions that one may search with advantage for cumulations, the units by which to build the new. There the evidence will be unobscured either by the pyrotechnics of Mendelian formulæ, or by the factitiousness of abnormal stimuli. Fluctuations will be present, but statistical methods will permit their evaluation. Should the measurement of the mean in the tenth or even the one hundredth generation present no advancement, failure is not necessarily implied. Nature has devoted fifty millions of years or more to her work. There should be no discouragement if a few paltry years of investigation fail in duplicating her methods.

It is with a feeling not unmixed with pessimism, however, that one views the conditions under which work of the character outlined must evidently go forward. Those engaged in teaching have with a few exceptions time for little more than an occasional investigation of limited scope, particularly in a field which requires continuous application. Governmental departments where it could best be taken to a successful issue have only too often been subservient to political policies which demand immediate results. An ounce of compiled compendium is—to them—worth more than a ton of painstaking investigations which makes an advance on a theory. Looking a few generations into the future is not their concern.[6] A remedy for such conditions clearly

[6] Exceptional work has been done by those more or less closely connected with certain State Agricultural Experiment Stations. The names of East and

lies in endowments either in connection with universities, or through the establishment of the specialized private institution.

That the problem of applied evolution will eventually be solved there can be no doubt. That it will occur in our generation may only be expressed as a hope.

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November 15, 1913

THE MUTATION MYTH

It has long been recognized both on the botanical zoological sides, that sterility is a notable characteristic of species crosses or true hybrids. Where species are nearer to one another their resultant cross is naturally less sterile than when their affinity is more remote. In the case of plants it is usually particularly easy to trace even slight evidences of previous hybridization in the sterility and abortive character of some of the spores or pollen. In contrast to hybrids, genetically pure species are characterized by pollen grains or spores, as the case may be, which are all perfectly developed. I have satisfied myself by a very extensive study of the spores and pollen of liverworts, mosses, ferns (including numerous genera of all the important families, isosporous and heterosporous), lycopods, selaginellas, quillworts, lepidodendroids, equiseta, cycads, ginkgo, conifers (including numerous genera of all the tribes), gnetales (all the genera) and many dicotyledonous and monocotyledonous angiosperms, that in good species the spores or pollen is invariably perfect morphologically, that is fully formed and having normal protoplasmic contents. Known hybrids on the contrary are characterized by a greater or smaller number of abortive spores, which have little or no protoplasmic contents. This conclusion is by no means new but the wide range of facts examined in the present connection adds very materially to its strength. It has been further noted that so far as morphological conditions are concerned, plants of genetic purity, even when grown under extremely abnormal conditions, as exotics in greenhouses, etc., have perfect spores or pollen. For example a conifer or a cycad from Australia or Japan, grown in the hothouse and producing its pollen in the winter season, still shows the grains normally developed morphologically, whatever may be their physiological inefficiency.

The bearing of the facts indicated in the paragraph above is of great importance in relation to the mutation hypothesis of De Vries. This distinguished Dutch plant physiologist, a little over a decade ago, published a series of observations and generalizations under the title of "Die Mutationstheorie." His notable offering was the statement that material of a species of *Œnothera* or evening primrose, referred by him to Seringe's *Œnothera lamarckiana,* found growing spontaneously near Hilversum in Holland, was producing annually new species or as he preferred to call them, elementary species. In 1904 Professor De Vries was invited to lecture in the University of California on his sensational discoveries. The lectures were edited and published later by the director of the Desert Laboratory of the Carnegie Institution of Washington, with the title of "Species and Varieties, Their Origin by Mutation." Dr. MacDougal thus became both in fact and figuratively, the "*vox in deserto clamantis,*" the baptist of the gospel of mutation. His exploits with the syringe in the baptism and production of new species of plants by intraovarial injections appear further to render his claims in this direction beyond question. As secretary of the Botanical Society of America and by his repetition and elaboration of De Vries's cultures of *Œnothera,* he has done unquestionably more than any one else to diffuse the doctrine of mutation in North America. It has in fact become so widely accepted on our continent, that it has in many instances

Hayes, of Connecticut, Pearl, of Maine, Emerson, of Nebraska, Dean Davenport, Rietz and Smith, of Illinois, are familiar to all interested in the application of the principles of evolution. One often conjectures, however, as to the extent to which some of the most valuable contributions are in reality "by-products" of investigations meeting the approval of the "Missouri" type of legislator.